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## BONDS OF UNION BETWEEN TROPICAL MEDICINE AND GENERAL MEDICINE[1]

IT is my wish that my first greeting and that my parting words should express my appreciation of the honor and the pleasure of visiting you here in Porto Rico. It is distinctly profitable to me to have this opportunity of exchanging ideas and experiences with you in our chosen field of work. In arranging for our conference this evening Dr. Lambert wrote to me suggesting that my talk ought to have a title, and he even ventured to hope that this idea would not come as too much of a surprise to me. So I have selected a subject that will permit us to wander where fancy leads, perhaps, who can tell, to one or two unexpected developments.

Our crowded activities grant us but little time for reflection, and it is easy to overlook points of contact between the adjacent fields such as tropical and general medicine. Inevitably, these two fields have exerted a profound influence on each other.

Strangely enough, the conception of tropical medicine is rather foreign to many individuals in the profession at home. The very name sometimes tends to frighten people away. To a few, it represents merely a curiosity, interesting but unimportant. One of your obligations will be to arouse still further the interest and support in New York City of your work here. One summer I met an elderly physician who had spent his life travelling in the tropics. He said to me: "Mercy, no, I stay on the ship. I never look at those patients ashore. Why should I clutter up my head with all that nonsense? If I ever had to treat those diseases I'd go somewhere and take a month's course in the subject." A few of the more serious minded deceive themselves into thinking that tropical medicine represents a real opportunity to make distinctive discoveries of new etiologic agents and new and important clinical entities almost without effort. Sometimes I feel that my friends almost take it to heart that the generosity of nature in tropical lands should cheat industry and so rudely violate the stern principles of no reward without great labor.

[1] A popular address delivered on February 22, 1927, as visiting lecturer at the School of Tropical Medicine, University of Porto Rico, San Juan, Porto Rico.

Suppose, however, that we want to choose as a model a disease of extremely intricate nature, the details of which have been worked out with exactness. It is best to turn to the tropics for an example. In the etiology of malaria we have not one but three distinct parasites very closely related; however, to the eye of the protozoologist they are readily distinguishable. The transmission of the disease from patient to patient is a complicated process. The sexual forms of the malarial parasite come into prominence. They leave their intermediate host, man, to take up a cycle of development in their definitive host, certain species of mosquito. After the completion of this cycle the mosquito is ready to set up new infections in man. The pathology of the disease is well-nigh completely understood. The diagnosis in patients is reasonably exact. The treatment, though not perfect, is marvelous compared to that of many bacterial infections. Lastly, prophylactic measures, though difficult, are quite feasible. The antimosquito campaigns have been enormously facilitated by Marshall Barber's ingenious introduction of Paris green for combating larvae.

We look to Sir Patrick Manson and to the British nation as the founders of tropical medicine. They took advantage of their natural opportunities. Great Britain with her extensive possessions has many physicians employed in foreign service and they all come back to one spot. The conditions in the United States are very different. We have only a few men who go to the tropics and they come home to scatter across a broad continent. A corresponding difference applies in the two countries to patients who are invalided home. With us, this works to the disadvantage both of the patient and of the subject of tropical medicine. In the absence of the incentive of stern necessity, interest has been intermittent and the development of the subject has been slow and arduous in the United States. When I wanted to learn something of the scope of your new undertaking I turned not so much to the American medical journals for information but to the literature from Great Britain. There have been some exceptions to this general lack of interest at home. Occasionally some of our institutions have devoted a number of years of work and considerable sums of money to purely tropical problems, meanwhile carefully avoiding any permanent responsibility in this field. There seems to be a willingness to profit by special opportunities arising in the field of tropical medicine accompanied by an unwillingness to offer continuous support to the necessities of the daily routine in this subject.

Clearly we can not model our schools after the British pattern. We must find somewhere compensating advantages in our own circumstances. Here in Porto Rico one sees the fascinating opportunity of making a complete study of a disease. The hospitals provide facilities for thorough clinical observation. The laboratories stimulate the desire to experiment in many theoretical directions. But we are not satisfied until satisfactory results have been obtained in the prevention of disease under varying conditions and the unforeseen but interesting disadvantages of actual work in the field.

The finger of erudition is often pointed at the term "tropical medicine." Some medical men seem to feel that they have fathomed the mysteries of this term when they learn that workers in the tropics have frequently made the statement that there is no such subject as tropical medicine. In one sense, I agree with this. Admittedly the phrase is one of convenience rendered necessary by the geographical separation of diseases. But the distinction is justified on scientific grounds. Fascinating fundamental principles are found in tropical medicine for which no counterpart exists in the diseases prevalent in cold climates. Thus, for all intents and purposes, the insect hosts of disease and even the protozoan infections take no real part in the life of the student of medicine in the temperate zones. Let me assure you, it is a difficult matter to find a satisfactory phrase for differentiating the medicine of cold climates from that of the tropics. "Internal medicine" does not help greatly. Largely in a complimentary sense I have been using the expression "general medicine." In so far as general medicine permits itself to forego the subjects of protozoology and entomology, it becomes an important branch of the larger field of medical sciences.

Well, this ought to convince you of my warning that we might be tempted to wander from any fixed topic. I will not in any way attempt to sum up the more striking achievements of tropical medicine but will emphasize only those features in which this subject and general medicine have exerted an influence upon each other. Indeed, as already indicated, we must close some of the most brilliant pages of medicine.

In my judgment, the most fundamental influence that tropical medicine has exerted in the field of medical sciences is to be found in the discovery of vitamins and the group of diseases sometimes designated as avitaminoses. You remember when we studied medicine, not so very long ago, beriberi was classed among the specific infectious diseases, with the reservation that some unknown toxin might play a more or less decisive rôle. We now know that neither of those factors plays any part whatever. The clinical and epidemiological data seemed at times in the past to offer almost convincing evidence of

the infectious nature of this disease. Many striking incidents occurred in a manner almost suggestive of some conspiracy in nature to conceal the facts. For example, a ship with its crew in apparently good health would call at a port where beriberi prevailed. Then after some days the disease would, so to speak, "break out" among the crew. Furthermore, if a mother suffering from beriberi nurses her own child she becomes a serious menace to the health of that child. Such children are prone to develop beriberi in an acute form that terminates suddenly in death.

But the Dutch investigators knew and had known for many years that beriberi was due simply to lack of proper nourishment. This discovery was made by Eijkman in 1890. There is an incident of a very human nature connected with Eijkman's production in chickens of experimental beriberi or, more accurately, polyneuritis. At the time he was not working on beriberi at all and chickens are very likely the last animal that he would have selected for the study of beriberi. The incident, as it was described to me, occurred in this manner. The animal quarters for these experimental chickens lay at a little distance from the laboratory alongside the hospital building. Very unexpectedly, the chickens developed polyneuritis to such an extent that the investigations that were in progress were seriously threatened. So Eijkman courageously decided that it was important to find out the cause of this polyneuritis. Then, to his surprise, the chickens promptly recovered so he was obliged to go back to his original problem only to be interrupted again by the recurrence of polyneuritis. Many observers would very justly have felt discouraged at this point. But Eijkman with remarkable skill succeeded in unravelling a mysteriously intricate network and revealed a clear chain of events occurring in logical sequence. The denouement came in this manner. In the hospital the patients were served with polished rice. For the protection of the patients, a rule was in force that any rice left on the patient's plates must be thrown out. In the laboratory the animal boy was given a small sum of money for the purchase of the cheaper unpolished rice for the experimental chickens. Now the Malays are very kindly in their disposition and the laboratory boy had a good friend among the orderlies in the hospital. It does not require much imagination to see that it was easy to provide a handsome diet of polished rice from the hospital for the chickens in the animal house nearby. There was no need wantonly to squander these valuable funds supplied for the purchase of unpolished rice. Now by mere coincidence, it happened that after the first outbreak of polyneuritis appeared, the orderly in the hospital went on vacation and the laboratory boy went back to feeding unpolished rice. The symptoms disappeared and the animals recovered with mysterious rapidity. Then when everything was going nicely, the orderly returned to the hospital and the chickens resumed their diet of white rice and with it the symptoms of beriberi returned.

Looking back at this distance, everything is beautifully clear. There is some strange substance in rice bran, in various grains, and in many other foods which is necessary for normal nutrition; in the absence of this substance polyneuritis develops. Eijkman picked his way with remarkable accuracy through this complicated maze in the face of many conflicting theories about beriberi. His conclusions were promptly rejected. The rest of the world said, "Oh, no, it just can't be true!" Twenty years later several investigators had the very happy thought of trying it. They made a discovery. They found that it is true. There is no flaw in Eijkman's experimental data, though his interpretation has subsequently undergone some revision.

Beriberi is not uncommon in the Philippine Islands. The Filipinos offered information to the profession which was not utilized. Patients under treatment sometimes reported that they could cure themselves with a diet of a certain native bean. Not knowing the facts about beriberi at this time, we explained to them very kindly the error of their ways. They accepted our explanations with gratitude and continued in their superstitious practice. Their confidence was not misplaced, that is, their confidence in dietary measures was justified.

Barely a quarter of a century after Eijkman's first publication, the physiologists began an accurate investigation of the requisites of a balanced diet. The first steps were difficult. It was necessary to give up some old established comfortable views. Proteins, carbohydrates and fats with a little salt and water had long been regarded as an adequate bread of life. Now it was rather disturbing to have to admit that some utterly unknown substance, even in minute quantity, exerts a powerful influence and is an essential item in our daily diet. Funk used the term vitamin to designate this substance which prevents the development of beriberi. As the interest in this phase of nutrition increased, other vitamins were discovered. Recently one has been described by Evans in California which is concerned not with ordinary nutrition but with the process of reproduction. In the field of medicine investigators naturally sought to explain other diseases on the basis of a dietary deficiency. It had long been known that scurvy is relieved by lime juice. Work of outstanding importance was accomplished by Goldberger in pellagra. Now in beriberi the problem is relatively simple; in

pellagra it is more complicated. Undoubtedly much valuable progress has been achieved in pellagra, but it is by no means certain that we have the complete story in hand as yet. It is not clear whether a vitamin is lacking or whether the deficiency lies in some other factor. The process of disproving the theory of a specific infection proved to be a long task even in the simple conditions of beriberi. One frequently hears of pellagrins whose symptoms do not yield to dietary measures. In this type of patient, it is particularly desirable to obtain additional evidence before utterly dismissing the idea of an infectious disease. In brief we are forced into a somewhat unwelcome situation; it is clear that a dietary disorder can produce symptoms sufficiently like those of a specific infection to cause long-standing confusion.

In the subject of etiology, let me mention first of all an interesting association between two diseases, that, let us say, seldom meet. One of them, namely sprue, is truly a product of the tropics. The other, progressive pernicious anemia, is a serious mystifying disease of cold climates. So far as we can tell at present, it occurs at least very irregularly in the tropics. Here in Porto Rico sprue is endemic. I intend to take full advantage of my opportunity to learn a great deal from you about this disease. You all know of the important work of Dr. Ashford on the rôle of monilia. As regards the causation of pernicious anemia, so little is known that one is fancy free to hold almost any view. It may be a specific infection, or perhaps it is a disturbance of nutrition or it has even been regarded as a disorder of the blood-forming organs analogous to malignant disease. With all the intensive study that has been made there is still very little to guide one along the correct path. Any suggestion would be very welcome. You may be interested to know that students of pernicious anemia are beginning to take an intense interest in sprue because of certain similarities which sometimes occur in these two conditions. I refer more particularly to anemia, to achylia gastrica and to the changes in the spinal cord. Here we have clearly a clue to be followed up and it is being followed by several investigators in the United States. Considerable work is now in progress on the occurrence of monilia in pernicious anemia. It is much too early yet to say what course this work may take, what it may develop into, or where it may eventually lead us. You see that we are only at the very beginning of our knowledge concerning the cause of pernicious anemia. But in the treatment of anemia very gratifying progress has recently been accomplished through dietary measures by Minot and Murphy. The very name of this disease, progressive pernicious anemia, is sufficiently terrifying and there is already ground for hope that we may be justified in dropping the word "progressive" from this ominous phrase. By means of a diet of liver, patients suffering from pernicious anemia have experienced relief over periods of one to two years in a manner that has not been accomplished by any previous form of treatment.

Now you all know that the old-established treatment of sprue rests on dietary measures. This type of therapy, though very new in pernicious anemia, is an old procedure in sprue. We have the strawberry diet, the milk diet and the meat diet. It is already time to consider the inter-application of dietary measures in the case of refractory patients afflicted with either of these diseases. To illustrate, some sprue patients improve best of all on a meat diet but can not stand the monotony of this régime. One would not hesitate to transfer such a patient to the diet of liver as used in pernicious anemia.

We are now in a position to consider for a moment the question of etiology. We are confident that an infectious process is an important factor in the production of at least some of the symptoms of sprue. In pernicious anemia, the relief of symptoms by a change in diet might be interpreted as evidence of a purely metabolic disorder. But the analogy with sprue must be studied further. As yet it would be premature to close our minds completely to the possibility of a specific infection in pernicious anemia. It would be a distinct advance if one could establish even the general type of disease to which this form of anemia belongs. The geographical distribution of sprue and pernicious anemia renders it difficult for one individual to familiarize himself with the two diseases; yet a mutual understanding of the two is helpful.

I believe it was Pascal who gave us the important counsel that the apparently trivial exception sometimes opens up the way to unsuspected fields of important information. In this connection let us remember that scarlet fever is not endemic in the tropics, but streptococcal infections occur here though perhaps with less frequency than at home. This leads us, in a minor way, to something of a paradox. The weight of opinion in the United States at present tends toward the acceptance of the streptococcus as the cause of scarlet fever. It has always been a puzzling question as to why scarlet fever does not occur in the tropics; if it is caused by a streptococcus the solution of this question becomes even more important. You are all familiar with the happy results that are being accomplished in the treatment of scarlet fever with serum, this work having been initiated in your affiliated school in New York by Dochez and by the Dicks in Chicago. Clearly there is an opportunity here in Porto Rico which does not exist in New York and Chicago to furnish supplementary evidence regarding

the etiology of scarlet fever. I assume that no one here or elsewhere in the tropics has yet studied anew the streptococci found in the tropics and compared them serologically with those isolated from scarlet fever. If the streptococcus found in scarlet fever occurs here, then one has the interesting task of discovering why it does not produce the clinical symptoms seen in cold climates. Cases of scarlet fever are imported here occasionally, but perhaps the streptococcus is unable to persist in virulent form in this climate. This would imply a subtle difference between this streptococcus and its close relatives. Very likely one might be able to find an analogy for these circumstances, but the situation is sufficiently interesting to arouse the imagination.

It is an easy matter to explain why some diseases are limited to the tropics. We have in but very few instances arrived at a plausible explanation to tell us why some types of infection are found only in cold climates. The subject of scarlet fever brings to mind the question of measles. It is always helpful to see the same disease under varying conditions. The characteristics of life in the tropics offer certain minor advantages in the very difficult problem of the etiology of measles.

We are all familiar with the world-wide distribution of bacterial diseases as a group; many species utterly disregard climatic conditions. The protozoa are very discriminating and their home is distinctly in the warm climates. Pathogenic amoebae do get something of a foothold in the north; but there is room for doubt as to how long they would maintain themselves there if the supply from the tropics were suddenly and completely wiped out. The spirochaetes in their biological characteristics occupy a position which, in one sense, is intermediate between the bacteria and the protozoa. Likewise, the geographical position of the spirochaetes follows an intermediate course. There are a few pathogenic species which thrive entirely independently of climatic conditions, but the pathogenic group taken as a whole shows a predilection for the tropics.

Long ago Schaudinn suggested that the cause of yellow fever would prove to be a spirochaete and Stimson demonstrated a spirochaete in the kidney in one patient dying presumably of yellow fever. The demonstration of leptospira as the causative agent of infectious jaundice added a fresh impetus to the search for spirochaetes in yellow fever. Noguchi worked intensively in this field. His results raise questions of fascinating interest regarding the relationship between yellow fever and infectious jaundice, *i.e.*, Weil's disease. The subject is an intricate one, and it will be best to reserve it for detailed discussion at a later period.

Let us turn to an example where our information concerning the main features of etiology is complete. It is one in which tropical medicine received very direct assistance from general medicine. Clinical analogies had long been recognized between yaws and syphilis and even over-emphasized. Following Schaudinn's announcement of the discovery of *Treponema pallidum,* Castellani very promptly supplied convincing evidence establishing a similar treponema as the cause of yaws. Some of you may not recall that the treponema of yaws was actually seen by Castellani before *T. pallidum* was described, but under such circumstances that Castellani did not appreciate its etiologic rôle. The subject of tropical medicine just missed the opportunity of pointing the way to the etiology of syphilis. Yaws and syphilis illustrate well the firm bonds of union between tropical medicine and general medicine. It is impossible to appreciate either of these diseases thoroughly without a comprehensive knowledge of the other. Yaws is one of the comparatively few diseases that is truly tropical and this geographical limitation is not dependent on an insect vector. It seems to me by no means fanciful to regard syphilis as an evolutionary change in yaws for its adaptation to cold climates. While the treponema of yaws is restricted to the tropics, it is indeed regrettable that nature has succeeded so well in adapting *T. pallidum* to the rigors of cold climates.

Thus the development of our knowledge of yaws owes a real debt to the general medical sciences not only as regards its etiology but also in the application of the Wassermann reaction. This debt has been in a large measure repaid in a way that is not fully appreciated. Chemotherapy of the systemic infections received during its infancy almost its sole impetus from the field of protozoology and the spirochaetal diseases. The development of salvarsan was very closely associated with tropical medicine. As you know, trypanosomes and the disease trypanosomiasis, *i.e.*, sleeping sickness, furnish a practical method for the study of chemotherapy. Ehrlich, by systematic investigation, tested his long series of compounds on trypanosomes and on various spirochaetes. It so happened that salvarsan proved to be effective for many spirochaetal diseases, including syphilis. More recently the Rockefeller Institute devised a chemotherapeutic agent, tryparsamide, for the treatment of trypanosomiasis. Some clinicians with an inquiring turn of mind tried the effect of this drug in the late stages of syphilis with results of decided interest.

There is another achievement in chemistry that we must not pass over. Under the conditions of tropical life, several important parasites can be demonstrated in the blood stream. A good staining technique is in-

dispensable. The German workers in tropical medicine did their part in developing the Romanowsky stain to the point where it has become the routine method which is used in one form or another throughout the world in the study of blood conditions. The modification devised by Giemsa has been applied by Wolbach in the study of sections, particularly for the demonstration of Rickettsiae in tissues. This technique has proved itself to be very valuable in attacking this interesting and difficult group of microorganisms.

Biochemistry has also made its contributions. Some years ago there was a small outbreak of Asiatic cholera in Manila. At that time it fell to my lot to be on duty in the cholera wards. Some of the patients in the stage of reaction showed unmistakable clinical signs of air-hunger, an almost typical Kussmaul's coma. Obviously these cases were not associated with diabetes, and the urine, as a rule, was free from acetone. However, the clinical signs of acidosis were characteristic and it seemed advisable to look for some method other than the tests for acetone bodies for the recognition of acidosis. Accordingly these patients were injected with sodium bicarbonate. Enormous quantities—90 or 100 grams—were often required to render the urine alkaline, whereas if a healthy person takes a teaspoonful of soda the urine changes promptly from an acid to an alkaline reaction. Formerly, a large proportion of all cases of Asiatic cholera, roughly 15 per cent., died of uremia. Now it was found that early treatment of cholera patients with bicarbonate practically eliminated the complication of uremia. Therefore it seemed probable that a similar lack of alkali might occur in patients developing uremia in the terminal stages of Bright's disease as we see it in cold climates. Accordingly, in Baltimore I examined such patients. They showed an even more intense degree of acidosis than the cholera patients, but obviously they were in the end stages of a long-standing disease and no lasting benefit could be expected from treatment with alkali. For our understanding of nephritis it is important for us to know that acidosis is one of the factors which is responsible for the symptoms of uremia. This fact is now generally accepted, for it has been confirmed by many observers using chiefly the method of direct chemical analyses of the blood—a method that in my opinion is rather less delicate than the test of tolerance to alkali.

These studies in Asiatic cholera therefore have given us an improvement in its treatment and a demonstration of a type of acidosis that differs markedly in detail from that of diabetes. This acidosis occurs not only in the nephritis of cholera patients but also in the ordinary nephritis of cold climates.

Before leaving the general field of chemistry, let us look kindly and briefly at a small and now harmless skeleton behind the curtain. An error of a pharmacological nature delayed the development of emetine therapy in amoebic dysentery for two decades. From time immemorial the natives of India had known that ipecac often, though by no means always, gave relief in the treatment of dysentery. With the differentiation of the amoebic and bacillary forms of dysentery, medical men became more and more interested in ipecac treatment. Now if any of you have ever had occasion to undergo the old ipecac treatment, you will know what a heroic undertaking it is to manage sixty or ninety grains of this drug, on account of the fearful nausea that ensues. In an honest effort to advance the use of ipecac, the idea was put forth that the drug does not owe its action to its content in emetine. For a time this conception gained ground. In my day in medical school we were advised to use de-emetinized preparations of ipecac in treating amoebic dysentery. It seems strange that this error was not corrected by the experience gained in actual practice. One possible explanation is that some of the presumably de-emetinized preparations still contained appreciable quantities of emetine. The correction of this error came about twenty years later in this manner. Vedder working in Manila found that emetine was very toxic for the cultures of the harmless water amoebae and he advised that the pure alkaloid be tried in the treatment of infections with *E. histolytica*. Rogers acting on this suggestion developed the treatment of amoebic dysentery by the intramuscular injection of emetine.

We must at least mention the topic of climatology. We may look forward to interesting advances in this subject in the future. Under natural conditions, it is very difficult to judge the effect of climate on man. There are many variable factors which are not susceptible of control and the effects are not susceptible of exact analysis. Accordingly, some wonderful laboratories have been equipped at home in which artificial climates may be produced and regulated at will. The problems are numerous; some physiologists are interested in determining the effect of climate on the nervous system and on the mental processes of the individual.

In matters pertaining to climate, I would rather turn to the infectious diseases. Radical theories have been advanced suggesting that infectious diseases may change profoundly under the influence of climate. Diseases may be limited by climatic conditions. In the main, they refuse to change their symptoms and

their general behavior. To illustrate, it is now accepted that syphilis and yaws are distinct diseases. We no longer hold the view that syphilis under tropical conditions changes its manifestations and gives rise to yaws.

Permit me to point out one very interesting organism, namely, *B. pestis,* the causative agent of bubonic plague. This disease is transmitted by the rat flea. But at times, especially in northern climes, this same bacterium *B. pestis* has given rise to a disease characterized not by buboes but by pneumonia and we have pneumonic plague. Occasionally, cases of bubonic plague also develop pneumonia, but epidemics of the pneumonic form occur only in cold climates. Insects play no part whatever in transmission, but droplet infection is the mode of conveyance. Droplet infection explains itself. Some of you may remember having been in countries so cold that on stepping out of doors in the morning you could "see your breath" on the air. It is this "breath" which carries microorganisms with it. The organism *B. pestis* is easily recognized in each of these two types of disease. Otherwise a strange error might easily have occurred. It would have been only natural to suppose that these two types of infection were totally unrelated diseases; one a pneumonia, always fatal, and epidemic in cold climates, the other a disease of rats and man, carried by fleas and characterized by buboes.

Let us now take up briefly some phases of epidemiology and hygiene. Manson really shocked his best friends by suggesting that filaria, a parasite found in the blood, might be transmitted by mosquitoes. Undaunted, this pioneer demonstrated in 1878 that certain species of mosquitoes take up filaria from the blood of a patient. Moreover, he followed developmental stages of the filariae in the mosquito. The manner in which the mosquito carries the infection back to man was not demonstrated for a period of many years. In the meantime it fell to the lot of Theobald Smith to furnish the first complete demonstration of "insect" transmission. As you know, he showed clearly that the disease Texas fever, which affects southern cattle, is transmitted by the cattle tick. In spite of very clear experimental evidence, this conception proved almost too daring for scientific acceptance by the leaders in medicine of that period. In the face of skepticism, the demonstration of the insect transmission of malaria, of yellow fever and other diseases soon followed. An incident occurred in Cuba which expresses very well the attitude in that day toward the insect transmission. You remember that Dr. Carroll, of the yellow fever commission, submitted to an experimental infection with this disease. When his infection was fully developed he told his nurse one evening that his attack of yellow fever was produced by the bite of a mosquito. During his convalescence, Dr. Carroll was looking through his clinical chart and he found this entry by the nurse, "Patient is delirious to-night; says he got his fever from a mosquito bite." Eventually a long list of infections have been shown to be insect borne. Indeed it might seem that an easy way was opened up to determine the rôle of insects in transmitting an infectious disease. The problem however is not quite so simple. I would call your attention to the stubbornness with which kala-azar has steadfastly refused to yield the secrets of its way of life in the insect world. It seems now, under the attack of the three commissions engaged for some years in field work, that kala-azar is almost proven to be transmitted not by fleas nor the lowly bedbugs but by sand flies.

One of the very few absolute triumphs in hygiene in the United States was accomplished by virtue of the demonstration in Cuba concerning the rôle of mosquitoes in yellow fever. This disease has now vanished from general medicine of our northern cities, probably never to return. A relative triumph in hygiene in our southern states owes its origin to the demonstration by Ashford and his colleagues here in Porto Rico that uncinariasis can be controlled by appropriate field measures. It challenges the imagination to conceive the far-reaching ramifications of this work that had its beginning here only a few years ago. Recently this work has received a fresh impetus by virtue of the remarkable advances made in Washington by Hall and his associates in antihelminthic therapy. The splendid development of the hookworm campaign throughout the world reached such proportions that the practical difficulties multiplied themselves. The introduction of carbon tetrachloride by Hall furnished a reviving influence to this campaign.

One could easily multiply the instances in which remarkable results have been achieved through public health measures in cold climates for the purpose of driving back to their home the diseases originating in the tropics.

Recently I have had the pleasure of visiting the department of health and the privilege of seeing something of the work of your director of public health. The achievements in hygiene in Porto Rico are progressing to such an extent that the workers in this institute will be driven not merely to neighboring islands but farther and farther from these shores on expeditions for research in its many and varied phases. There are advantages and disadvantages about these expeditions. The one thing we can be sure of is that they are a necessity.

Obviously the opportunities for teaching here are splendid. I hope it may be your privilege to bring about a closer association in work, in thought and in

the exchange of ideas between the staffs of this school and that of your affiliated school in New York. Perhaps you may enjoy the distinction of seeing these two branches of medicine grow into one subject. Fortunately you do not have the responsibility of providing here a complete course in general medicine. Our schools at home are carrying a burdensome curriculum that is constantly growing. But think of the situation which pertains, for example, in the University of the Philippines. There the students must be given a thorough foundation not only in general medicine but they must also be prepared to meet the daily problems in protozoology, helminthology and entomology. After all, the ideal location for a school of general medicine is in the tropics. Such schools have not as yet attained the distinction of a genuinely international reputation. The opportunity for the time being is lying dormant.

We have sketched very lightly some of the more obvious ways in which the interests of tropical and general medicine are intermingled to form the growing structure of the medical sciences. We have illustrated this by a consideration of vitamins and their relationship to beriberi, scurvy, rickets, pellagra and to the physiology of nutrition and reproduction. You have before you now the important results which have just been achieved in the study of rickets by the commission from the Yale Medical School. The relationship of sprue and pernicious anemia commands special interest here in Porto Rico. The study of the streptococci in the tropics will aid in advancing our knowledge of scarlet fever. Among the spirochaetal diseases there is much opportunity for reflection. We have the treponema of syphilis and yaws and the leptospira of Weil's disease and yellow fever. In the field of biochemistry, progress has been made in the study of nephritis as it occurs in Bright's disease and in Asiatic cholera. Turning from medicine to hygiene, we find in many respects a common interest in principles, and the necessities of travel and commerce bring about a closer association in practice. In the experience of the individual these relationships can as a practical matter be but little more than points of contact between the medicine and hygiene of these distant zones. As we look more closely we find firm bonds of union between the medical problems of lands that lie always in the summer sunshine and those accustomed to perpetual fog.

Little did the physiologists dream that a fundamental discovery in nutrition would originate in the small island of Java. Nature has lavishly endowed this island of Porto Rico. Its stimulating freshness lends inspiration for work and for ideas. The richness of your chosen subject defends it against monotony. Here it will be easy and natural to follow the precept recommended by Professor Williams at the founding of the Sigma Xi. He said in part, "In kindling your torches we bid you light them at the brightest living altars of learning and not at the smouldering embers of dead issues." As the years slip by, many students will look back with satisfaction on the incentive received in this favored place.

It is a matter of importance to the scientific world that the people of Porto Rico have achieved a definite consciousness of their responsibilities in science. The leaders in the development of this island are not satisfied merely with commercial progress. The activities of your investigators have given Porto Rico a place of leadership in science in tropical America. This is an enviable position which in time will be challenged by your neighbors in friendly rivalry. But with the foundation of past achievement and with mature plans and preparations for the future, it is a leadership which Porto Rico is in a position to maintain.

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## NOMINA CONSERVANDA

AN article in the November, 1926, issue of the Proceedings of the Entomological Society of Washington by Mr. W. L. McAtee, entitled "Nomina Conservanda from the Standpoint of the Taxonomist," shows such an astonishing failure to grasp the relations that exist between nomenclature and taxonomy that I can not permit it to pass without protest.

"Why do scientists," queries Mr. McAtee, "most of whom presumably are evolutionists, attempt to block development in taxonomy while constantly accepting change in other fields both within and without the domain of science?" The inference is that adherents of the idea of nomina conservanda must answer to the charge of being obstructionists. The answer is that they do not attempt to block development in taxonomy; no such charge could be made by one who understood the function of the rules of nomenclature.

Taxonomy concerns itself with the classification of organisms, and modern taxonomists accept the principle that classification must express, as nearly as may be, organic relationship. In a word, taxonomy must as closely approximate the phylogeny of organisms as the state of our knowledge makes possible. It is therefore a science, and, like all sciences, is dependent upon our knowledge of facts and our interpretation of the significance of the facts we know. It would be intolerable to have it codified or ruled upon by any group of individuals, however organized, for it is the bounden duty of every man of science to make known the facts of science as he perceives them,

and it is his inalienable right to interpret those facts according to his own best judgment.

The nomenclature of organisms is, on the other hand, a matter of language. It is a tool that the taxonomist must use, and use well, in common with all other zoologists, too. A central body can regulate it, and should do so, for only in so far as it is standardized and universal is it useful, and in so far as it is individual is it not a tool at all, not a language, but babel. Mr. McAtee may prefer to babel names that no one else understands and has the inalienable right to do so, but if I, on the other hand, wish to apply my names in a manner that has been standardized by reasonable central authority and therefore make them intelligible to others, Mr.McAtee may not imply that my course is less progressive or that I am, in so doing, obstructing scientific progress.

The rules of nomenclature never attempt to settle the status of organic groups. Neither they in general nor nomina conservanda in particular settle or rule upon matters of scientific fact nor the interpretation of those facts. Given one hundred individuals, Mr. McAtee may interpret them as one hundred species and one hundred genera—one hundred families if he likes and has the inalienable right to do, nor will any ruling of nomenclature or any proposition of nomina conservanda prevent it, any more than it would prevent me from considering, if I so chose, that they were all one species. The rules of nomenclature say that *if* one accepts a certain group of organisms as having the status of species, subspecies, genus or what not he shall apply to them such and such a name, and they also provide what name he shall use if he change that status or accept them as of another status, or if he dissociate them from a group with which they have been previously combined or combine them anew with others. About what course the taxonomist shall follow in all these matters the rules of nomenclature are silent, for it is none of their concern.

Just that fact is the reason why the hope that any rules of nomenclature could or should afford a permanent stability in all cases is futile. Such an expectation is based on ignorance. Even theoretically they can only attain a nomenclatorial stability in so far as taxonomy remains stable.

If I to-day call species z and y both members of the genus A-us, and to-morrow decide that they are not, no rule of nomenclature can nor should prevent prevent me from then calling the one A-us z and the other B-us y, a change of name corresponding to the change of taxonomic status. If to-day I call two individuals both species z and to-morrow I do not, no rule of nomenclature can nor should prevent my assigning a new name to one of them. If I assign ten genera to one family, and Mr. McAtee assign them to ten families, no rule of nomenclature nor no nomen conservandum can nor should prevent his act, nor all the changed family names under which those organisms would thereafter be ranked, but if he follow his course (or I mine) the rules may prescribe what names we must use.

All rules of nomenclature must provide for unlimited change, corresponding to changed taxonomic concepts, and they do. To this nomina conservanda are no exception.

A nomen conservandum does not attempt to set up a status quo, thereby dictating for all time that a name shall be used for a group of specified limits. It does not specify the limits of a group for any time; no rule of nomenclature does so. They are not concerned with limits; for limits are questions of fact, or of judgment—not arbitrable, belonging to taxonomy. Just because it can *not* deal with limits nomenclature can only deal with types. It can only define a genus as all those organisms which any given taxonomist accepts as congeneric with the type species. It proclaims, and only proclaims, that now and for all time all those species[1] which any given taxonomist considers as congeneric with a specified type species shall by him be called by a specified generic name. It equally provides that any taxonomist, not considering some of these as congeneric with the specified type, shall not use that generic name in combination with them.

Therefore, given the Genus A-us, type species z, the principle of nomina conservanda may provide at one and the same time that Mr. Blank, accepting species x as congeneric with z, shall use the combination A-us x, and that Mr. Brown, not accepting species x as congeneric with z, shall *not* use the combination A-us x; which is right and as it should be.

Mr. McAtee goes on to say, "Certainly there is no real value in preserving a familiar name unless it embodies a definite concept. Proponents of nomina conservanda assume that these names do embody such concepts, but this is a fallacy. In fact, the longer a name has been in use the more we may be assured that authors have applied[2] it to diverse organisms.

[1] So far the principle of nomina conservanda has only been applied to generic names. If extended to specific names, or to family or other group names the principle would be identical, except that no principle of nomina conservanda could tolerably be applied to a combination of generic and specific names, other than for the name of a genus and that of its type species, for that proposition would instantly involve the limits of genera, and therefore taxonomic decisions.

[2] Misapplied would be better. It is the duty and purpose of rules of nomenclature to clear up and prevent such misapplications. But Mr. McAtee may mean cases

If Mr. McAtee will substitute the modern concept of a taxonomic group as its type and all other organisms that any given zoologist accepts as properly belonging to the same group for the old idea that a group consists of all organisms that come within its original definition he will see that the limits of a group may be as variable as the number of taxonomists who study it, but that its *nucleus* must remain fixed. With that understanding the force of the quoted sentences and of those that follow withers.

Mr. McAtee continues with his confusion between taxonomy and nomenclature:

> "The definite concept idea is not retroactive . . . Furthermore the definite concept idea has no anticipatory value, for we can not be insured against future change. . . . Taxonomy is dynamic not static, and its development demands never-ceasing perfecting of analysis and definition. Setting up nomina conservanda is attempting to establish fixed entities in a field where change, where progress, necessarily has been the rule. It amounts to fixing limits to the search for knowledge. . . ."

If the name A-us is a valid name for a generic group, consisting of species z as type and others, and ten different authors have used ten different generic names for groups in which they included (as type) species z all since the original proposition of the name A-us, obviously the definite concept idea is not retroactive in the sense that it can alter the fact that they have done it, but it is retroactive in the sense that it can proclaim that from our standpoint these were misapplications not to be followed. And it is anticipatory in the sense that it can proclaim that for all future time that particular organism and any others that the future may include with it as congeneric, if any, can only be termed the genus A-us. Taxonomy *is* dynamic and not static, but we must have an intelligible language for it; and our nomenclatorial system provides for unlimited flexibility, change, progress. A nomen conservandum differs from any other name only by the fact that for especial reason, by common agreement we have decided that a particular name E-us shall apply to the type of a genus and its accepted congeners (accepted by any given worker—not by any pronouncement) instead of any other name as A-us, which the rules might perhaps otherwise validate. Its effect never is to limit a generic concept, but only to fix a nucleus and a name; its result is therefore to stabilize and standardize nomenclature, but not to limit the search for knowledge. There is no "sacrifice of scientific ideals of evolution in methods and of progress in knowledge." There can not be, for these things are not involved.

"Furthermore they [*i. e.,* nomina conservanda] can be established only by nullification of the fundamental principle of nomenclature, prority." Why is priority fundamental and how far? Only because it has proved a useful tool and only so far as it is a useful tool. "Priority" is a convention to be discarded just at the point where it begins to impede instead of helping.

The generic name Crabro has been universally used in literature for a common and well-known group of aculeate wasps, in accordance with a usage introduced by Fabricius in 1775. It has escaped all authors (until it was pointed out in 1919) that this name had been used by Geoffroy in 1762 for a well-known group of saw-flies, universally since his day called Cimbex. By the law of priority we are hereafter obliged to call the saw-flies Crabro instead of Cimbex, and to call the wasps by some other less familiar name. That is an exceedingly confusing and awkward thing to have to do. It is not helpful in the case of these names; it is an abomination. But the writer and others who feel with him believe that it is better to accept an abominable situation and make the best of it in an occasional instance like this than it is on his own authority or that of any individual to abrogate the law of priority, which common agreement and experience has shown to be a useful tool. If we can by common agreement through a representative body decide selectively in such special cases that it is more useful to abrogate the principle of priority (or any other convention) than to follow it, we are relieving an abominable situation, and proving ourselves masters of our tools instead of slaves to them. That process is what we call establishing nomina conservanda. It is not a bogey to be afraid of. It is an act of common sense. The danger is only when individuals attempt to establish them by their own unsupported acts; for by their very nature they are useful and tolerable only by common agreement and adoption.

The next argument enunciated by Mr. McAtee is that the actuating purpose of a taxonomist's work is to build himself a monument and that the institution of nomina conservanda threatens the names that the taxonomists originate and which are to be their monuments. I can not read that paragraph without growing angry. The taxonomist who works to build himself a monument had better turn to another field: if

of organisms once supposed to be identical, but which with the lapse of time and the growth of knowledge are now known to be diverse—or supposed to be. In such instances the misapplication would only become such after the diversity was known, and by one who accepted the diversity as a fact. Neither case invalidates the idea that nomina conservanda apply to a definite conception —namely a type and all other organisms accepted by any given zoologist as of the same group.

his reward does not come from the joy of discovering new truths and relations and helping others to discover them, his work is more likely to be an impediment to progress than a useful thing. Taxonomy has suffered too much in the past and fallen too far into disrepute, from the petty work of persons infected with the "mihi itch." Were it not a bibliographical necessity—or so considered—it would be *far* better to not cite the name of an author in conjunction with a scientific name and to forget who proposed it. At least the sooner it is understood, the better off we will be, that we do *not* include the name of the author as part of the formal name of an organism in order to give him "credit," but as a matter of bibliographic record. If it must come to a question of a monument to posterity, there are those who would prefer to leave taxonomic work that would win the approbation of specialists for its sound judgment of phylogenetic relationships, for its scholarliness and helpfulness, even though it never proposed a new name, than to have coined names for a thousand genera and species, each flaunting the describer's name like a waving ensign to dazzle the uninitiated, who may not know how easy and insignificant a thing it is to propose a new name or describe an avowedly new form.

"Taxonomists originate the names, work with them more than other scientists, and in all ways have greater interests in them, and rights over them." As a taxonomist I protest against any such point of view, or against Mr. McAtee thinking that he speaks for "entomological taxonomists almost to a man." The language of zoology is the common property of all zoologists. If the taxonomist allows himself to become so sunk and enmeshed in his own limited group that he can not see, or disregards the needs of the non-specialist in that group for an intelligible nomenclature of it, if he fails to meet the legitimate needs of the general zoologist, of the morphologist, of the ecologist, he may expect that the general workers will ride rather roughly shod over him, for they will not tolerate hampering of their progress in a field that should contribute only cooperation and facility.

The principle of nomina conservanda is sane, sound common sense, when properly applied. It permits us to use the rules of nomenclature up to the point where they serve a useful purpose and to abrogate them just at that point where their further employment would be an unquestioned detriment. The "plenary power" resolution of the Monaco Congress gave the International Commission power to suspend the rules in any given case where in its judgment the strict application of the rules will clearly result in greater confusion than uniformity. It is like the executive clemency principle, which recognizes that in individual cases greater injustice may be done by application of the law and its penalties than by their suspension. But unlike executive clemency it is not subject to political considerations or to individual motives. It must be the unanimous[3] act of an international board of experts—the only representative body of zoologists that exists. There have been very few cases in which this power has been used; and that the commission will be conservative in its future application may be taken for granted. No individual or other body has any recognized right to establish a single nomen conservandum. That this power now exists in the International Commission is a cause for congratulation, a progressive step; a sign that we are to be bound by convention and rules only to the point where they serve a useful purpose, and are not to allow ourselves to become their slaves.

J. CHESTER BRADLEY

CORNELL UNIVERSITY

## SCIENTIFIC EVENTS

### A CODE OF ETHICS FOR SCIENTIFIC MEN

THE Committee on Social and Economic Welfare of Scientific Men, appointed at the Phoenix meeting of the Southwestern Division, American Association for the Advancement of Science, presented the following tentative code of ethics for discussion at the Santa Fe meeting of the division, April 13, 1927. The code was unanimously adopted.

(1) Assume an obligation to do honest work and to impartially present the same to the public, regardless of political, economic or religious prejudice, pressure or tradition.

(2) Exemplify in your conduct and work a courageous regard for the whole people, and not alone some powerful and influential fraction thereof with which you come in close personal contact.

(3) Recognize and assume a dual obligation (a) to do the best possible work in your field, (b) to promote the social and economic welfare of your colleagues and yourself.

(4) Promote the dignity of your profession; avoid malicious criticism of colleagues; cultivate a professional consciousness.

(5) Support laws to insure competency and high standards on the part of scientific-technical men in every field.

(6) Respect yourself and your profession; do not underbid your colleagues; insist that the laborer is worthy of his hire.

(7) Be slow to change jobs and institutions where such a change means a loss of project efficiency, but do not

[3] A two-thirds agreement results in final decision by a special committee appointed by the succeeding International Congress of Zoology.

hesitate to change where the attitude of autocratic superiors, miserably inadequate pay or other conditions conducing to inexcusable inefficiency menace the entire service you are endeavoring to perform..

(8) Investigate before accepting a new position; do not become a candidate for any position from which the previous incumbent was unfairly or wrongly dismissed, or a position in any institution under the ban of dignified organizations of scientific men.

(9) Insist on such a measure of reasonable participation in the determination of policies in your institution as will best promote effective scientific work.

(10) Do not publish the work of colleagues or subordinates without giving full credit where credit is due; authorship should be determined on the basis of the responsibility for the ideas involved, conception and organization of the project, actual field or research work, and actual compilation and writing of the results.

(11) Avoid, alike, hasty and superficial publication, and the holding of real results indefinitely without publication.

(12) Take the public into your confidence; in the end the public pays the bills and has a right to know what is going on.

(13) Interest yourself in human concerns outside your specialty—politics, religion, economics—your obligation to serve the community along these lines is directly proportional to your training and real ability.

The Committee on Social and Economic Welfare of Scientific Men is composed of the following members: Byron Cummings, acting president, University of Arizona, *chairman;* Frank E. E. Germann, University of Colorado, Boulder; G. A. Pearson, Southwestern Forest Experiment Station, Flagstaff; Walter P. Taylor, U. S. Biological Survey, Tucson. The committee will cooperate with the Committee of One Hundred on Scientific Research of the general association in its work for the advancement of research and research workers.

## THE WORLD LIST OF SCIENTIFIC BIBLIOGRAPHY

THE World List of Scientific Periodicals published by the Oxford University Press has been completed. The London correspondent of the *Journal* of the American Medical Association writes:

Few as large, and certainly no more arduous, tasks in bibliography have ever been accomplished. The first part of this great undertaking was to compile in alphabetical order a list of all periodicals containing the results of scientific research in existence between the years 1900 and 1921. This was published as volume 1 in 1925. It contains the stupendous number of just over 24,000 separate periodicals. But the list was not complete, notwithstanding the exhaustive search of known catalogues made by Dr. Pollard, then keeper of printed books at the British Museum, and in a supplement issued with volume 2 more than 600 periodicals have been added. The preparation of the second volume necessitated even greater labor and has performed an even more important service to science. The adage "verify your references" is made difficult, sometimes impossible, by the ambiguous abbreviations of titles often given by authors. To overcome these difficulties, several institutions have adopted their own sets of abbreviations. These, however, are for the most part based on a limited series of periodicals, and also differ among themselves. The second volume of the World List consists in the first place of a set of abbreviations consistent and unambiguous for the whole set of nearly 25,000 periodicals. If it could be universally adopted, the temporary inconvenience of changing existing systems would soon be overcome by the permanent advantage to all scientific workers. Even when a reference is given correctly, the seeker after knowledge has to discover where he can find the periodical. To aid in that, twenty-one centers in Great Britain and Ireland have been selected. Symbols have been assigned to the more important libraries in each of these centers, and after the contraction for each periodical is placed the symbol of libraries in which the periodical is to be found. But apart from the direct aid supplied in this way, some remarkable and disconcerting results have appeared, because for a considerable proportion of the periodicals no home in Great Britain and Ireland has been discovered. Even London, with twenty-seven important scientific libraries, misses many publications of high value.

## A CENSUS OF WATER-FOWL

A MONTHLY census of water-fowl at selected points throughout the United States is being planned by the Biological Survey of the Department of Agriculture. It will be an aid in administering the Federal migratory-bird treaty act and the regulations thereunder, for the protection of birds that migrate between the United States and Canada. The undertaking is for the purpose of obtaining accurate information regarding the numbers, distribution and migration of water-fowl throughout the United States, Canada and Mexico. The project is important not only to the country as a whole and to each of the states, but also to all organizations that are primarily concerned with the conservation of game, all sportsmen and all others interested in wild fowl or their conservation.

In carrying out this projected work the Biological Survey plans to establish as many volunteer observation stations as possible, particularly in areas where there is great concentration of water-fowl in winter or during migration. In addition, it is desired to gather all possible information regarding the numbers and distribution of our water-fowl during the breeding season. On the selected areas accurate counts or estimates of ducks, geese, swans and coots are to be made throughout the country each month on the same designated dates. When the numbers of birds

are small enough actual counts will be made, and otherwise, estimates of their numbers. Accuracy in these counts and estimates is insisted upon as of prime importance to the purpose of the work.

It is hoped by these censuses to learn not only more than has before been possible to know of the numbers of the ducks, geese, swans and coots, but also additional facts regarding their distribution and their migration routes. By repeating the observations during succeeding years it will be possible to determine whether these birds, so important to the sportsmen and to the country at large, are actually increasing or decreasing. It will also throw light on the causes of local fluctuations that often are puzzling. Each census taker is urged to select the area of great concentration in his locality and one that can be conveniently covered in a single day or a portion of a day.

This project will be inaugurated during the coming August. Cooperation is assured from various agencies of the United States Government, including the National Park Service, Lighthouse Service, Coast Guard, Bureau of Fisheries, Bureau of Reclamation, Office of Indian Affairs, Bureau of Education, and the Forest Service, Weather Bureau and Extension Service of the United States Department of Agriculture. Cooperation has been invited from sportsmen, ornithologists and other interested organizations and individuals.

## THE STUDY OF EPIDEMIC ENCEPHALITIS

DR. WALTER TIMME, chairman of the joint finance committee of the trustees and medical staff of the Neurological Institute at the new Medical Center, New York City, recently made the announcement that to promote research study and treatment of encephalitis J. P. Morgan has made a gift to the institute of $200,000 to be used for the construction and equipment of a complete hospital floor containing forty-eight beds. The gift was designated as a memorial to Mrs. Morgan, who died of the disease.

The Morgan fund places at the disposal of the medical profession facilities for investigating sleeping sickness and will enable the institute to bring to bear upon this problem the combined resources of the entire Medical Center, now in the process of completion at 165th Street and Broadway.

That the disease of this country and England differs from that found in Africa was pointed out recently by Dr. Aldo Castellani, discoverer of the germ of that disease, who came to this country to organize a department of tropical medicine at Tulane University.

So wide is the territory covered by the malady in its varied forms that it has been apparent for some time to the medical authorities that an international survey of all expressions of the so-called sleeping sickness would be the only logical method of determining the extent of the germ's range.

Mr. William T. Matheson has provided funds to pay the cost of a survey of encephalitis in this country, Europe and possibly Asia. A commission has been formed with Dr. William Darrach, dean of the College of Physicians and Surgeons of Columbia University, as chairman. The commission includes Dr. Frederick Tilney, professor of neurology in Columbia University; Dr. Hubert Howe, instructor of neurology, secretary; Dr. Haven Emerson and Dr. Frederick Gay, who are both on the faculty of the same college as professors of public health administration and bacteriology, respectively, and Dr. W. J. Park, director of the bureau of laboratories of the New York City Health Department and professor of bacteriology of Bellevue Hospital Medical College. Direction of the research program will be under the supervision of Dr. Josephine B. Neal.

## THE ROCKEFELLER FOUNDATION

IN his review of the work of the Rockefeller Foundation, Dr. George E. Vincent, the president, states that during 1926 the foundation, in disbursing $9,741,474 (1) aided the growth of fourteen medical schools in ten different countries; (2) maintained a modern medical school and teaching hospital in Peking; (3) assisted the development of professional public health training in fifteen institutions in twelve countries and in ten field stations in the United States and Europe; (4) contributed to nurse training schools in the United States, Brazil, France, Poland, Jugoslavia, China, Japan and Siam; (5) sent, as emergency aid, journals, books or laboratory supplies to institutions in twenty European countries; (6) helped twenty-one governments to combat hookworm disease; (7) gave funds to organized rural health services in 244 counties in the United States and to thirty-four districts in twelve other countries; (8) shared in the creation or support of various departments in state or national health services in sixteen countries; (9) cooperated with Brazil in the control of yellow fever, or in precautionary measures against the yellow fever mosquito, in ten states; (10) continued yellow fever surveys and studies in Nigeria and on the Gold Coast; (11) aided efforts to show the possibilities of controlling malaria in nine North American states and in Porto Rico, Nicaragua, Salvador, Argentina, Brazil, Italy, Spain, Poland, Palestine and the Philippine Islands; (12) helped to improve the teaching of physics, chemistry and biology in eleven institutions in China and in the government university of Siam; (13) supported the Institute of Biological Research of the Johns Hopkins University and con-

tributed toward the publication of *Biological Abstracts;* (14) gave funds for biological or mental research at Yale University, the State University of Iowa and the Marine Biological Station at Pacific Grove, California; (15) provided, directly or indirectly, fellowships for 889 men and women from forty-eight different countries, and paid the traveling expenses of sixty-nine officials or professors making study visits either individually or in commissions; (16) helped the Health Committee of the League of Nations to conduct international study tours or interchanges for 120 health officers from forty-eight countries; (17) continued to aid the League's information service on communicable diseases; (18) made surveys of health conditions, medical education, nursing, biology or anthropology in thirty-one countries; (19) lent staff members as consultants and made minor gifts to many governments and institutions; (20) assisted mental hygiene projects both in the United States and in Canada, demonstrations in dispensary development in New York City, and other undertakings in public health, medical education and allied fields.

## SCIENTIFIC NOTES AND NEWS

At a *conversazione* held by the British Institution of Electrical Engineers, on July 7, at the Natural History Museum, South Kensington, the president presented to Dr. Elihu Thomson, honorary member of the institution, the Faraday medal which had been awarded to him by the council.

At the fifth centenary of the founding of the University of Louvain, on June 29, honorary degrees were conferred on Dr. Simon Flexner, director of the Rockefeller Institute; on Edward Dean Adams, the electrical engineer, and on Alfred Douglas Flinn, secretary of the American Engineering Society, New York.

At the commencement exercises of the University of Porto Rico held in San Juan, Dr. Juan Iturbe, of Caracas, Venezuela, received the honorary degree of doctor of science.

The president and council of the Royal Society have recommended Mr. Stanley Baldwin, the British prime minister, for election into the society under the special statute which permits the election of persons who, in their opinion, either have rendered conspicuous service to the cause of science or are such that their election would be of signal benefit to the society.

Professor Sir Edward A. Sharpey-Schafer presided over the section of physiology and biochemistry of the British Medical Association meeting in Edinburgh during the latter part of July.

At the annual election of fellows into the council of the Royal College of Surgeons of England, Sir Berkeley Moynihan presiding, there were nine candidates for three vacancies. The following was the result of the election: Sir Cuthbert Wallace (489 votes) and Mr. William Thelwall Thomas, M.B.E. (475 votes), were reelected, and Mr. Hugh Lett (366 votes) was elected a member of the council.

The German Society for Cancer Research on July 4 gave a dinner in honor of Dr. Frederick L. Hoffman in the Zoological Garden, following an extended address by Dr. Hoffman on the utility of statistics in cancer investigations. The presiding officer was Dr. Kraus, president of the society. Among those present were Dr. Otto Warburg, Dr. Hamel, president of the Federal Health Department, Dr. Krohne, minister of public welfare, Dr. Blumenthal and others.

Dr. Edward Kremers, professor of pharmaceutical chemistry in the University of Wisconsin, has resigned to organize and direct research in colloid chemistry for E. I. du Pont de Nemours and Company, at Wilmington, Delaware.

Theodor Theodorson, of the Johns Hopkins University, under the auspices of the Oil Heating Institute, has been placed in charge of research work on the process of combustion in an oil flame. Mr. Leod D. Becker, managing director of the institute, states that this is the first time that a well-coordinated investigation into the air-fuel ratio, shape and size of combustion chamber, possible draft variations and methods of mixing fuel in domestic oil burners has been planned by non-commercial authorities.

Horace S. Isbell, who has been working on organic gold compounds for the United States Public Health Service at the University of Maryland, has resigned to accept a position as associate chemist with the Bureau of Standards, Washington, D. C.

Dr. Ella Woods has resigned her position as assistant professor of home economics in the University of Wisconsin in order to take charge of the research work in home economics under the Purnell Grant at the University of Idaho.

At the recent meeting of the trustees of the Beit Memorial Fellowships for Medical Research, Dr. H. H. Dale, head of the department of biochemistry and pharmacology of the Medical Research Council, was appointed a member of the advisory board in succession to the late Professor E. H. Starling.

Dr. B. T. Dickson, professor of botany at McGill University, Montreal, has been appointed by the Australian government chief mycologist of the Council of Scientific Industrial Research.

Dr. C. L. Huskins, of the University of Alberta, who has been carrying on his researches on the cytology and genetics of fatuoid oats for the last two years in the botanical department at King's College under Professor R. R. Gates, has been appointed to a research post in the John Innes Horticultural Institution at Merton.

To complete the unfinished work of Dr. Charles D. Walcott, late secretary of the Smithsonian Institution, on the stratigraphy of the Rockies, a motor truck expedition to the mountainous northwest has been dispatched from the institution. The expedition is under the direction of Dr. R. S. Bassler and Dr. Charles E. Resser. Its first destination is Utah, whence it will work up through Montana to British Columbia.

Dr. F. G. Banting has left Toronto for Sydney, Nova Scotia, with the intention of accompanying the Canadian Government's annual Arctic expedition in the steamer *Beothic,* which is sailing for the Polar regions.

Professor Leo E. Melchers, head of the department of botany and plant pathology of the Kansas State Agricultural College, has been appointed by the Egyptian Ministry of Agriculture to do some special work in plant pathology for the Egyptian Government. He sails on September 3 from New York to Italy and then to Alexandria. He will be on a leave of absence for a year.

Dr. E. P. Churchill, head of the department of zoology of the University of South Dakota, is in charge of a party making an ecological study of the fishes of South Dakota; the investigations this summer are to be concerned largely with the waters of the eastern half of the state.

Dr. Leo Wolman, lecturer at the New School for Social Research and economic adviser of the Amalgamated Clothing Workers of America; Dr. Elwood Mead, agricultural expert of the United States Reclamation Service, and Professor Jacob G. Lipman, of Rutgers University, will proceed shortly to Palestine to join the staff of experts there in a survey of the economic possibilities of the country.

We learn from the *Journal* of the American Medical Association that an expedition under the auspices of the University of Cincinnati has gone to Mexico to study the use, under clinical conditions, of an active vaccine treatment for typhoid, which Dr. William B. Wherry, professor of bacteriology and preventive medicine at the university, has given a preliminary trial on several cases of typhoid in Cincinnati. In addition to Dr. Wherry, there is in the party Mrs. Wherry, who is also a physician; Dr. Thomas J. Le Blanc, associate professor of preventive medicine; Dr. Lee Foshay, assistant professor of internal medicine, and Robert Thomas, a junior medical student. The party sailed from New York on June 30, and will be in Mexico for about three months. Arrangements have been made assuring the cooperation of the Mexican health officials.

Dr. Arthur M. Banta has returned to the Station for Experimental Evolution of the Carnegie Institution of Washington after a leave of absence for the spring quarter during which he gave a course of lectures at the University of Minnesota on genetics and eugenics. He also gave some other lectures while at the university, including a short series of conferences before a group of geneticists and others on the "Genetics of Cladocera."

Dr. Edward J. Menge, head of the department of zoology at Marquette University, has sailed for South America, where he will make an extensive lecture tour for the next two and a half months. Dr. Menge has been invited to lecture at the three oldest universities on American soil, Lima, Peru; Cordova, Argentina, and Sucre, Bolivia, as well as at other universities. He will speak on modern trends of biological work.

Sir Thomas Oliver, the British authority on occupational diseases, will sail for the United States with Dr. Frederick L. Hoffman, from Cherbourg, on August 13. After a few days in Quebec and Montreal, he will leave for Boston and Wellesley Hills, proceeding later on a tour of inspection of industrial plants in Niagara Falls, Chicago, St. Louis, Pittsburgh, Philadelphia, etc. Sir Thomas may possibly deliver several addresses on industrial problems while in the west. He expects to be about four weeks in the country.

Dr. Henry Mills Hurd, emeritus professor of psychiatry at the Johns Hopkins University and until he resigned in 1911 superintendent of the Johns Hopkins Hospital, died on July 20, at the age of eighty-four years. Dr. Hurd had been editor of *The American Journal of Insanity, The Johns Hopkins Bulletin, The Johns Hopkins Medical Reports* and *The Modern Hospital.*

Professor Charles Fuller Baker, formerly dean of the Agricultural College of the University of the Philippines, died on July 21, aged fifty-five years. Professor Baker was the brother of Ray Stannard Baker, the author, and Hugh Potter Baker, the forester.

Dr. Arthur A. Hamerschlag, president of the Research Corporation and previously director of the Carnegie Institute of Technology, died on June 30, in his sixty-fifth year.

WILLIAM PAUL GERHARD, consulting civil and sanitary engineer of New York City, died on July 8, aged seventy-two years.

DR. WILLIAM O. KROHN, the alienist of Chicago, died on June 17, aged fifty-nine years.

DR. MAGNUS OLOF MITTAG-LEFFLER, until 1911 professor of mathematics in the University of Stockholm, died at Djursholm, Sweden, on July 11. Professor and Mrs. Mittag-Leffler have bequeathed all their property, including the mathematical library and their estate at Djursholm, to an international mathematical institution, which has already been established and bears the name Makarna Mittag-Lefflers Matematiska Stiftelse.

THE death is announced of Professor De Bruin, one of the foremost pediatricians in the Netherlands. His principal works were studies on infantile scurvy and on cerebrospinal meningitis. He was one of the founders of the Netherlands Pediatric Society.

THE trustees of the Field Museum of Natural History have voted to dedicate the museum's Hall of African Mammals as the "Carl E. Akeley Memorial Hall," in honor of the explorer, sculptor, inventor, taxidermist and founder of museum methods, who died in the Belgian Congo on November 27, 1926.

SOME extra copies of the portrait of the late Arthur Bolles Lee, the author of "The Microtomist's Vade-Mecum," which was issued with the last number of the *Journal of Pathology and Bacteriology,* are available and may be had by any one interested on application to the editor at 17 Loom Lane, Radlett, Herts, England.

THE sixth International Congress of the History of Medicine is being held at the University of Leiden and at Amsterdam from July 18 to 23. In Amsterdam there has been organized for the occasion an important exposition of paintings and books pertaining to anatomy and the art of healing.

NEXT September will be held the triennial congress of the International Institute of Anthropology. The Netherlands National Bureau of Anthropology, of which Dr. Kleiweg de Zwaan is the president, is entrusted with the organization of the congress. At the same time, the International Federation of Eugenic Organizations will convene in Amsterdam. The papers will be divided among the following sections: (1) physical anthropology, (2) ethnography and ethnology, (3) heredity and eugenics, (4) sociology and criminology, (5) the prehistoric period and (6) folklore. Dr. Charles B. Davenport, director of the Carnegie Laboratory for Experimental Evolution and of the Eugenics Record Office at Cold Spring Harbor, will speak on the crossing of races.

THE British Association will meet at Leeds, for the third time, during the period August 31 to September 7. *Science Progress* writes as follows: "The first meeting took place in 1858, a few weeks after Wallace and Darwin had read their papers on the origin of species, and Sir Richard Owen's presidential address formed the opening note in the long controversy which has raged round that subject. In the twenty-two years which elapsed before the next meeting the Yorkshire College had achieved fame and two of its professors acted as sectional presidents. The meeting this year will be the first since the college has developed into the University of Leeds. It promises to be unusually interesting, and will be notable for the first appearance of a woman as sectional president. Sir Arthur Keith, the president, has taken as the title of his address 'Darwin's Theory of Man's Descent as it stands To-day,' and Professor Whittaker, president of Section A, will deal with 'The Outstanding Problems of Relativity.' The evening discourses will be given by Professor R. A. Millikan (Cosmic Rays) and Dr. F. A. E. Crew (The Germplasm and its Architecture). Among the discussions which have been arranged are those on 'The Structure and Formation of Colloidal Particles' and on the 'Climates of the Past.' There will be the usual receptions, excursions to the Dale country, a garden party at Fox Hill (by the invitation of Colonel C. H. Tetley, pro-chancellor of the university) and special meetings for the discussion of textile problems."

A SERIES of lectures is being given at the Mount Desert Island Biological Laboratory as follows:

July 18, Dr. Harlow C. Shapley, professor of astronomy, Harvard University, on "Concerning World Evolution"; July 25, Dr. Edwin G. Conklin, professor of zoology, Princeton University, on "Some Common Misconceptions regarding Evolution"; August 1, Dr. Herbert S. Jennings, professor of zoology, the Johns Hopkins University, on "Biological Fallacies and Human Affairs"; August 8, Dr. Roy W. Miner, curator of invertebrates, The American Museum of Natural History, on "Exploring a Coral Reef from the Bottom of the Sea"; August 28, Dr. Kirtley M. Mather, professor of physical geography, Harvard University, on "Science and Religion—Friends and Enemies."

APPLICATIONS for associate physical chemist must be on file with the Civil Service Commission at Washington, D. C., not later than August 9. The examination is to fill vacancies in the Bureau of Chemistry and Soils, Department of Agriculture, and in positions requiring similar qualifications. The entrance salary in the departmental service at Washington, D. C., is $3,000 a year. A probationary period of six months is required; advancement after that depends

upon individual efficiency, increased usefulness and the occurrence of vacancies in higher positions. For appointment to the Field Service the salary will be approximately the same. The duties in the Bureau of Chemistry and Soils will be to conduct research studies and technical investigations pertaining to fires in farm products, with special attention to spontaneous combustion and deterioration of hay, grain, cattle feeds and other agricultural products, and the development of methods for their control and prevention. Competitors will not be required to report for examination at any place, but will be rated on their education, training and experience, and a publication or thesis to be filed with the application.

*Industrial and Enginering Chemistry* reports that representatives from the Museum of the Peaceful Arts of New York City and from the Smithsonian Institution of Washington, D. C., of which the National Museum of Engineering and Industry will be a part, recently attended a luncheon in connection with the annual meeting of the latter organization. The intimation was given that owing to changes which were contemplated in the plans of the Mall in Washington by the Commission of Fine Arts of that city, there possibly would be a change in the location of the site of the museum building on the Smithsonian grounds. At a subsequent meeting of the commission attended by representatives of the Smithsonian Institution and the National Museum, a new site satisfactory to all the parties interested was agreed upon. At the meeting after the luncheon above referred to, officers and trustees for the current year were unanimously elected as follows: *President,* Thomas Ewing, former commissioner of patents; *Secretary,* Harrison W. Craver, director, United Engineering Societies Library; *Trustees,* L. P. Alford, B. C. Batcheller, George M. Bond, Nicholas F. Brady, Ericsson F. Bushnell, Fred H. Colvin, F. A. Halsey, Thomas T. Hoopes, D. C. Jackson, Joseph Keller, Fred R. Low, H. P. Merriam, H. F. J. Porter, Dr. M. I. Pupin, Dr. Elmer A. Sperry, Kirby Thomas and F. A. Waldron.

THE Associated Press reported on July 13 the lake of lava from Kilauea crater, which began an eruption on July 7, was at that time steadily building to new levels on the floor of the eight-mile-wide Halemaumau pit as the lava from the cones spreads in spirals about the hardening surface. The principal cone, as the tube of hardened lava about each center of eruption is called, is about fifty feet high. It is continually capped with a layer of rock which hardens from its molten state, except for the periodic outbreaks when the accumulated pressure from the subterranean forces thrusts itself through and sends out a fiery fountain. The flow of other fountains below the surface of the lake which covers more than 100 acres on the floor of the Halemaumau pit can be plainly seen by the motion of the crust, which occasionally breaks to permit new flows to spread. R. M. Wilson, volcanologist, predicts that the lava lake will gradually rise until the fifty-foot cone is submerged, after which the flow of lava will continue beneath the surface. The flow yesterday was as strong as at any time since the eruption began.

## UNIVERSITY AND EDUCATIONAL NOTES

PLANS are in preparation for a laboratory of physics to be built for the Johns Hopkins University at a cost of $350,000.

THE Sheffield Scientific School of Yale University has received by the will of Chester W. Lyman, formerly president of the International Paper Company, the sum of $50,000, to be used in teaching hydraulic engineering and allied subjects.

DR. HARRY YANDELL BENEDICT, professor of applied mathematics and dean of the college of arts at the University of Texas, has been elected to succeed Dr. Walter Splawn as president of the university.

DR. ROLLIN T. WOODYATT, of the University of Chicago, has been made chairman of the department of medicine.

PROFESSOR EDWIN D. STARBUCK has been appointed head of the department of philosophy at the State University of Iowa. Philosophy and psychology, which have existed as a single department, are now separated. Professor Starbuck has also been officially made director of the Institute of Character Research which has hitherto been known as the Research Station in Character Education. The institute has received a special appropriation from the state legislature.

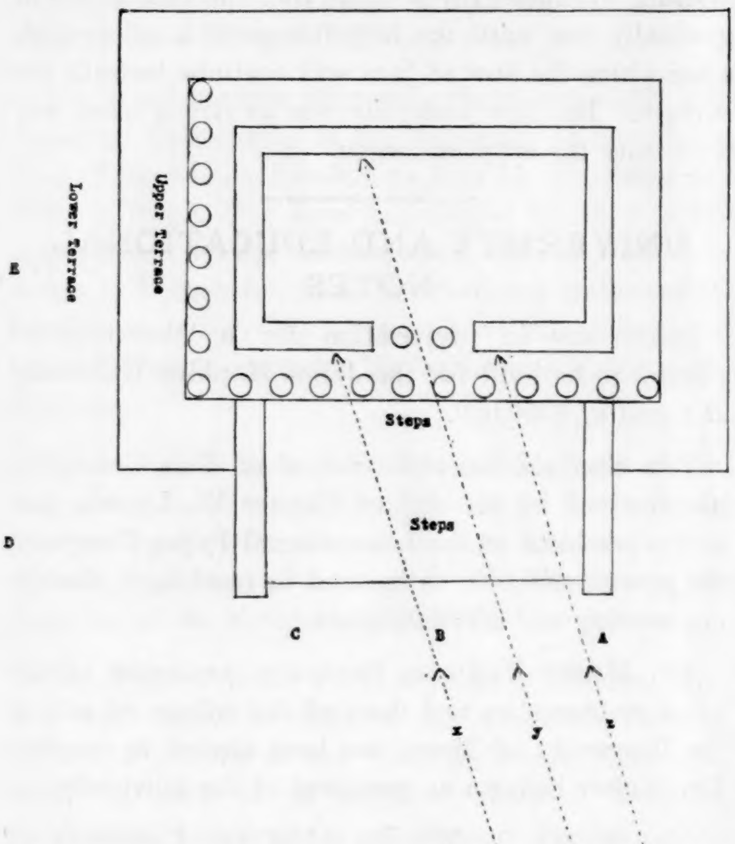
DR. IRVING W. BAILEY, associate professor of forestry at Harvard University, has been appointed professor of plant anatomy.

## DISCUSSION AND CORRESPONDENCE

### THE VARIABLE ECHOES PRODUCED BY THE LINCOLN MEMORIAL

ON the evening of June 11 during a display of fireworks on the executive grounds in Washington, the writer's attention was drawn to the peculiar echoes produced by the scattering of sound waves against the outer and inner walls and the fluted columns of the Lincoln Memorial. In the accompanying diagram the direction of the sound waves, from a source

three quarters of a mile distant, is indicated by the dotted arrowed lines x, y, z. In the position A, at the bottom of the main steps of the approach, no echo was apparent. But as the observer approached




the point B a faint weird echo of high pitch began to be heard after each report of the exploding fireworks. As the observer continued towards the point C the intensity of the echo increased, its tone being of a shrill metallic quality, somewhat prolonged and resembling the sound produced by a file when drawn across the teeth of a saw. As one proceeded around the corner of the memorial towards the point D on the lower terrace the echo gradually changed to a lower pitch of more prolonged duration, the sound at this place resembling the sharp tearing noise produced by ripping a piece of cloth. As one passed onward towards the rear of the memorial the echo grew constantly fainter and ceased entirely at about the point E.

C. A. Browne

## THE LUNELL HERBARIUM

Botanists interested in the taxonomy of the flowering plants have long been familiar with specimens collected by J. Lunell, of Leeds, North Dakota.

Born in one of the well-known castles of Sweden, in 1851, where his father was rector, Dr. Lunell emigrated to the United States at about 37 years of age, bringing with him the responsibility for a family of three children.

After a year devoted to the practice of medicine in St. Paul, he felt the irresistible call of the frontier and took up his work at Willow City, North Dakota, in 1889, at a time when cities were but names which expressed the hopefulness and ambition of those who were living in dugouts, sod houses or board and tar paper shelters. He remained there for about five years before taking up his permanent residence at Leeds, North Dakota. From the first moment of his arrival he began to collect and study the plants of the region. As a student, his leisure time had been devoted to the collection of plants, all of which were left behind when he came to the United States. Demands for the services of the one doctor often crowded out eating and sleep, but if the long outgoing journeys permitted no opportunities for delay, the return trips always afforded a means for noting and collecting plants of particular interest. It is a great misfortune that there were not more such men to study and preserve for future reference actual specimens of a flora which has now largely disappeared through the ravages of fire and the inroads of agriculture.

While Dr. Lunell is known widely to taxonomists through his collections, and his systematic botanical notes and papers, perhaps few are aware that he was a man of highly varied interests. Graduated from the University of Upsala, he read Latin, Greek and Hebrew as well as the modern languages. Before coming to America, in addition to the translation of technical writings, he had made some of the writings of Mark Twain, Marryat, Savarin and other French and Russian authors available to those whose reading was limited to the Swedish language. His volumes of classical music, well worn by use at his own piano, were about as numerous as the bound botanical works of his small library.

Since Dr. Lunell's death, at sixty-nine years of age, in 1920, his herbarium has been little used by botanists. It is unfortunate that there is not now more local interest in collections of the plants and animals of the various regions of the United States, but until such local interest exists, it is desirable that collections of this kind, made by those when fired with enthusiasm for scientific work, even under difficult conditions, be ultimately assembled in centers where they can be available to students.

Students of the flowering plants will be interested to know that the Lunell herbarium has been purchased by the board of regents of the University of Minnesota for the department of botany. In the course of a few months, the materials will be incorporated in the herbarium and there be available to students who may wish to use them.

J. Arthur Harris

## INDEX KEWENSIS

I UNDERSTAND that it is not generally known that the sixth supplement of the Index Kewensis was published last year by the Clarendon Press. This includes references to the names and synonyms of genera and species of flowering plants which were published during the five years 1916–1920, and also includes many which had appeared in previous years in publications which, owing to the war, were not available at Kew.

The Index Kewensis is an indispensable work to all plant systematists, whether botanists or horticulturists, who desire to keep abreast of botanical nomenclature. The original index we owe to the generosity of Charles Darwin and six quinquennial supplements have now been published. Some idea of the labor involved in keeping up this record may be gathered from the fact that the sixth supplement recently published contains some 30,000 references.

With such increases in the number of new species and binomials, especially in such genera as Rosa, Rubus and Hieracium, the work of the systematist would be almost impossible without this periodic gathering together of the newly minted currencies in the world's botanical nomenclature.

As I have been informed that many sets of the Index Kewensis in botanical and horticultural libraries appear to be incomplete, and that in some cases supplements have been purchased for libraries which do not possess the original volumes, I have been asked to direct attention to the importance of the work. I would also point out that it is necessary, in order to keep abreast of botanical nomenclature, to possess all the supplements which have been published as well as the original index.

Copies of the original index or of any of the six quinquennial supplements may be obtained from the secretary, The Clarendon Press.

ARTHUR W. HILL

## THE USE OF THE GENERIC NAME WILSONIA

IT has recently been called to the writer's attention that there exists a duplication of the generic name *Wilsonia.* Priority of use seems to rest with Bonaparte's genus of wood warblers described on page 23, "Geographic and Comparative List of Birds, 1823" (cited by Ridgway, page 703, United States National Museum Bulletin 50, Part 2). In 1873 Keyser applied the name to a brachiopod previously called *Terebratula wilsoni* Sowerby. Keyser's description may be found on page 502, Volume 23, *Zeitschrift der Deutschen Geologischen Gesellschaft*, 1873. Rules of nomenclature, therefore, seem to demand that the name be reserved for the avian genus, and it is suggested that the next available name, *Uncinulina,* used by Bayle for the same form on Plate 13, figures 13–16, "Explication de la Carte France, Atlas," Volume IV, 1878, be used to replace the genus *Wilsonia* among the brachiopods.

W. C. TOEPELMAN

UNIVERSITY OF COLORADO

## A NOTE ON THE HISTORY OF ANTHROPOLOGY

SEVERAL inexplicable errors occur in the opening paragraph of President Henry Fairfield Osborn's paper on "Recent Discoveries relating to the Origin and Antiquity of Man" (SCIENCE, 65 (1927), 481). The "renowned Hans Virchow" is represented as opposing "the recognition of the Neanderthal skull of 1846 with pathologic and theologic preconceptions." The great pathologist's name was not Hans, but Rudolf; the Neanderthal skull was not discovered until 1856;[1] and any one acquainted with the psychology of that most tough-minded of scientists can only express a hope that all writers on the origin of man were as free from theological preconceptions as Virchow. It is true that theologically inclined writers were fond of citing Virchow's authority against Darwin, but his own position in the matter was skeptical to the point of negativism, not tinctured with any form of religious bias.

ROBERT H. LOWIE

## PROFESSORSHIPS IN MEDICAL SCHOOLS

IN the number of SCIENCE for July 15 I note in the delightful sketch of Dr. Franklin P. Mall, written by Dr. William T. Councilman, the statement that "all the teaching positions in medical schools throughout the country, with the exception of the chair of physiology at Harvard, were held by men who were active practitioners of medicine as well, and the professorial positions were regarded as valuable adjuncts to a medical practice."

The time referred to, I take it, is the early '80's. I would like to call attention to the fact that certainly since 1831 the occupants of the chair of anatomy in the school of medicine of the University of Pennsylvania have not been practitioners of medicine, and that certainly from 1818 and possibly earlier, this also holds true for the professors of chemistry in the same school. Certainly Leidy, who held the chair of anatomy from 1853 to 1891, and Theodore G. Wormley, who held the chair of chemistry and toxicology from 1877 to 1897, devoted their entire time to teaching and research.

WILLIAM PEPPER

[1] H. F. Osborn, "Men of the Old Stone Age," 3rd edition 1918, 217.

## QUOTATIONS

### THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE

Nearly a quarter of a century ago, on January 19, 1903, a small group of scientific investigators in New York met at the call of Professor Graham Lusk and the late Dr. S. J. Meltzer to consider the organization of active workers in experimental biology and medicine. This was the meeting that initiated the Society for Experimental Biology and Medicine under the presidency of Dr. Meltzer. The main object of the organization was the cultivation of the experimental method of investigation in the sciences of experimental biology and medicine. Membership in the group was limited to persons who had completed some meritorious independent experimental research in that field of study. The programs, from the start, consisted in brief presentations of the essential features of experimental investigations, and frequently of demonstrations of actual experiments. The membership soon outgrew its confinement to "Greater New York" and spread throughout the country. One reason for the success that has attended the development of this society lies perhaps in the significant circumstance that it has aimed to bring together workers in many fields, such as physiology, biochemistry, biology, bacteriology, pharmacology and experimental medicine, at a time when rapidly developing specialization had already begun to segregate investigators into small groups. The new society thus represented a wholesome reintegrating force and provided a stimulus for the discussion of "borderline" and interrelated problems. It has become an influence tending to overcome the danger of narrowness in the present-day outlook on the natural sciences with which medicine is so closely bound up. Another early object of the society was the development of high standards of presentation and scientific criticism. Incidentally, not a few significant discoveries have been announced for the first time at its meetings. As might be expected, this movement was bound to be followed by similar endeavors elsewhere. Many of them have resulted in the organization of branches of the society. To-day ten branches are located all the way from New York to Peking; eight more are at present under contemplation. The contributions, in the form of brief, concise summaries, are embodied in the *Proceedings of the Society for Experimental Biology and Medicine,* which is available through subscription. This journal deserves the active support of members of the medical profession, who are likely to find it stimulating and informative. Published without special endowment and maintained by the contributions of scientific workers, it needs and enlists the help of those who can benefit by its program.—*The Journal of the American Medical Association.*

## MUSCLE, YEAST AND CANCER CELLS

In his comparative study on the carbohydrates and gaseous metabolism of isolated muscle, at rest and at work, Otto Meyerhof[1,2] finally established beyond all doubt the doctrine that utilization of oxygen by muscle takes place normally, not during the act of contraction but rather during the periods of relaxation and rest immediately following. It was further shown that the energy required for contraction is directly derived from the breakdown of glycogen into lactic acid, whereas during the recovery period the oxidation involves a twofold action, the burning of one part of sugar, or its lactic acid equivalent, to carbon dioxide and water, while three to six times the amount of lactic acid is built back to glycogen. In other words, the immediate source of the energy for contraction is gotten by an anaerobic reaction, while the recovery from the contraction in its normal and most efficient manner is accomplished by an aerobic chain of reactions which culminates in the saving of a large part of the carbohydrate that had been split during the anaerobic phase or the contraction period. A further study revealed the fact that the processes found to hold for muscle in action also take place during periods of complete normal rest, although with much less intensity, so that the resting level of lactic acid concentration, from 0.015 to 0.03 per cent. of the muscle's weight, represents not only the residue of a previous recovery period but also the continuous balance sheet of a never-ceasing anoxidative carbohydrate splitting and an equally continuous oxidative removal of the split bodies by the twofold process of one part burned to carbon dioxide and water and from three to six parts built back to glycogen.

It is obvious that the anoxidative phase of these events is an expensive, prodigal one, but one apparently capable of yielding quickly and abundantly the free energy that is needed to enable the muscle cells to raise tension and to contract as quickly as they do; whereas the oxidative phase is one that not only frees the cells of the split products that accumulate during the anoxidative phase, clearing the decks for the next action, as it were, but does it in the manner of a salvager, rescuing at the same time as much of

[1] Meyerhof, O., "Die Energie Umwandlungen im Muskel," *Arch. f. d. ges. Physiol.*, *182*: 232, 284 and *185*: 11, 1920; *188*: 114, and *191*: 125, 1921; *195*: 22, 1922; Meyerhof, Lohmann, u. Meier, "Synthese des Kohlehydrats im Muskel," *Biochem. Zeitschr.*, *157*: 459, 1925.

[2] Hill, A. V., u. Meyerhof, O., "Über die Vorgänge bei der Muskelkontraktion," *Ergeb. d. Physiol.*, *22*: 299, 1923. (This joint review should be consulted for earlier literature and contributary evidence.)

the material as may be for future use. It will be noted that the breathing of the muscle cells is bound up with the aerobic phase only. Since the anaerobic splitting depends upon the action of ferments, this phase has been referred to as the phase of fermentative breakdown; the aerobic is often referred to as simply the respiratory phase.

With this cycle of alternating fermentative breakdown and respiratory recovery established for the skeletal muscle cell, the question arose as to whether the phenomena involved represented properties peculiar only to muscle or whether they were properties of other living cells as well.

Since the work of Pasteur the so-called anaerobic character of certain organisms has been known. Certain varieties of the yeast plant thrive amazingly well in the absence of air. Indeed the bottom wirts of the highly cultivated beer and vinous yeasts have been regarded as utilizing no oxygen whatever. The energy for growth in these cases is obviously derived from the anoxidative splitting of higher carbon compounds to carbon compounds of lesser complexity. On the other hand, yeasts such as the press-yeast, bakers' yeast and the wild yeasts continue to grow in the presence of oxygen, although their fermentation capacity at the same time is reduced. The problem of the part played by oxygen in the growth and fermentative action of yeast therefore had been recognized and studied not only by the more modern students of fermentation but also by Pasteur. But as Meyerhof points out, a definitive answer to the question could not have been obtained by the use of the cruder methods these workers had at their disposal; an employment of the more modern methods of micro-analysis such as were used in the study of muscle metabolism, however, ought to yield a less conflicting body of data. Such an application Meyerhof and his associates[3] have now made to the study of a number of various fermentation bacteria.

In these studies it is shown that even in the cases of the so-called anaerobic forms there is an actual utilization of oxygen, although small in amount, whenever this gas is admitted to the wirt in proper media. But however small or large the amount of oxygen utilization is, whether the form studied is of the "aerobic" press-yeast or bakers' yeast, or whether the form is of the "anaerobic" races, for example, the bottom wirt of beer and vinous yeasts, the same two processes of metabolism found for muscle cells are here also found. One process, the anoxidative, concerns itself with the fermentative breakdown of carbohydrate into ethyl alcohol, pyruvinic acid, acetaldehyde, lactic acid, acetic acid, etc., the other process, the oxidative, concerns itself with the complete oxidation of a part of the carbohydrate and the rescuing of another part from the fermentative breakdown. But more than this, when the ratio of the total number of molecules of split products removed to the number of molecules oxidized is determined for the various forms of bacteria, the oxidative quotient, as it is called, is shown to be of the same magnitudes as were found in the case of muscle, that is, between 3 and 6. In other words, for every molecule of carbohydrate oxidized to carbon dioxide and water 3 to 6 molecules are rescued from the fermentative breakdown. This, then, explains why the amount of fermentation products is less when oxygen is admitted to fermenting yeasts, and at the same time demonstrates that fermentation bacteria are only different from muscle in their metabolism chiefly in that the two major processes of metabolism have each undergone transformation (Umstimmung), the fermentative process having been greatly augmented, the oxidative salvaging process having been greatly depressed or partly lost.

As to how this transformation may have come about has been largely answered by both O. Warburg and O. Meyerhof and their associates in a series of independent studies. It was found by the latter[4] that so little as .0002 normal hydrocyanic acid present in the media of aerobic races of yeast reduced their oxidative power nearly 90 per cent., but their anoxidative fermentative power only as little as 10 per cent., and that by successive cultures these yeasts so transformed would produce permanent anaerobic strains of the plants. On the other hand, by treating anaerobic races with substances that stimulate their breathing capacity, permanent strains of aerobic plants could be cultivated. With increased utilization of oxygen increased salvage of carbohydrate was ensured and thus a corresponding decrease in the apparent fermentative splitting resulted.

Preceding and contemporary with these investigations on yeast two series of studies, one by O. Meyerhof and his coworkers,[5] the other by O. Warburg

[3] Meyerhof, O., "Über den Einfluss des Sauerstoffs auf die alkoholische Gärung der Hefe," *Biochem. Zeitschr.*, *162*: 43, 1925; and *Die Naturwissenschaften*, Jahrg. 14, p. 1175 (Dec.), 1926; also, Meyerhof und Finkle, "Über Beziehungen des Sauerstoffs zur bakterialen Milchsäuregärung," *Chem. d. Zelle u. Gewebe*, *12*: 157, 1926.

[4] Meyerhof, O., "Über den Einfluss des Sauerstoffs auf die alkoholische Gärung der Hefe," *Die Naturwissenschaften*, Jahrg. 13, p. 980 (Dec. 4), 1925; "Über den Zusammenhang der Spaltungsvorgänge mit der Atmung in der Zelle," *Ber. d. deutschen chem. Gesellsch.*, Jahrg. 58, p. 991, May, 1925.

[5] Meyerhof u. Lohmann, "Über Atmung und Kohlehydratsumsatz tierischer Gewebe," *Biochem. Zeitschr.*, *171*: 381, 421, 1926; R. Takane, *ibid.*, *171*: 403, 1926.

and his coworkers,[6] demonstrated that the two metabolic processes, anoxidative-fermentative-splitting and oxidative-salvaging of the split carbohydrate, were characteristics of animal tissue cells other than muscle, and especially of growing tissues, such as mucous membrane, skin and glandular tissues, and curiously enough also of retinal tissue. All embryonic tissue showed a high rate of both the fermentative splitting and the oxidative salvaging processes.

When pathologic cells, such as carcinoma cells, were studied by Warburg and his associates, the remarkable facts were revealed that these cells exhibited an excessively high rate of fermentation, causing a vastly greater amount of sugar to be split to lactic acid than normal cells do, but on the other hand a greatly diminished power of utilizing oxygen. Indeed it was found that cancer cells could live and continue to split sugar in a nutrient solution in which the pressure of the oxygen had been reduced to 1/100000 vol. per cent.; the cells could live and recover completely if asphyxiated thus as long as forty-eight hours. Longer asphyxiation, however, proved to be fatal. It was shown that they utilized oxygen in small amount independently of high tension of the gas. That is, the oxygen utilization as in normal cells appears to be determined only by the physiological capacity to breathe. The high rate at which tumor cells are capable of splitting dextrose to lactic acid is shown by the fact that *in vitro*, in an hour, they can produce an amount of lactic acid equal to 10 per cent. of their own weight. It is emphasized that in these quantitative studies the weight of tumor cells always refers to masses of pure tumor cells and not the usual mixtures of tumor and non-pathological cells that are removed by the excisions of the surgeon. Experiments testing the blood passing into and that passing out of a tumor in the living animal supports the results obtained *in vitro*. The blood from a tumor vein is found in rats to have two to three times as much lactic acid as that from an artery.

These physiological experiments with normal and pathologic animal cells when compared with those on muscle and yeast show, as the authors point out,[4,6] that there is a striking similarity in the physiological transformation that tissue cells must undergo to become tumor cells on the one hand, and that the aerobic yeasts must undergo to become the anaerobic type on the other. That normal embryonic cells may be transformed into cancer cells by injury *in vitro* (arsenic and other poisons) has been shown by Carrel and by A. Fischer. The cells so injured when injected into chickens developed sarcomas. The injury in these cases consisted first in damaging the respiratory function of the cells, but being embryonic cells they already had their fermentation capacity highly developed. When injected into normal animals the cells were able to thrive by continuing their anaerobic fermentation, their supply of sugar being furnished by the host. That they could utilize little oxygen was rather an advantage than a disadvantage to them. Warburg points out that these results confirm the view long held that cancers are the result of injury to normally growing cells and that the injurious agent may be of manifold variety.

It would seem that the next step in the field of cancer research is to find out if possible how the cancer cell can be transformed back to the normal cell; how it may most easily be trained to improve its breathing capacity, and if possible to reduce its fermentative capacity. If such retransformation can be effected metastases would be rendered much more unlikely, for during the early stages of fixation comparative oxygen-want and diminished ability to get energy from anaerobic fermentative carbohydrate splitting would more likely lead to the death of the cells. Surgical removal of parent tumors then would mean more certain the removal of all the tumor-producing tissue, and in cases of the inoperable kind one could still hope to check the growth.

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[6] Warburg, Posener, u. Negelein, "Über den Stoffwechsel der Carcinomzelle," *Biochem. Zeitschr.*, *152*: 309, 1924; Warburg, "Über den heutigen Stand des Carcinomproblems," *Die Naturwissenschaften*, Jahrg. 15, p. 1 (Jan. 7), 1927.

## SPECIAL ARTICLES

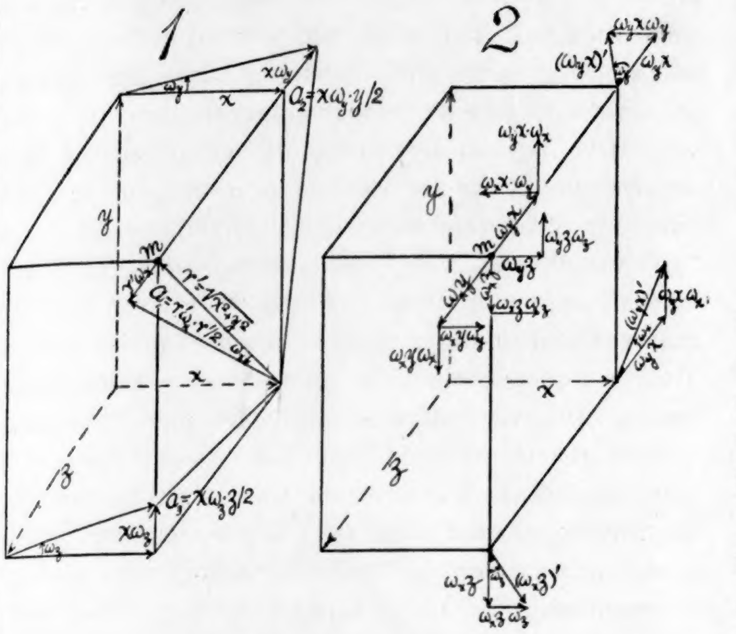
### GRAPHIC TREATMENT OF EULER'S EQUATIONS

I WAS struck, some time ago, with the ease with which the familiar Eulerian tensor ($A = \Sigma m(y^2 + z^2)$, etc., $D = \Sigma myz$, etc.)

$$\begin{array}{ccc} A & -F & -E \\ -F & B & -D \\ -E & -D & C \end{array}$$

usually reached analytically, can be written down from the mere inspection of an orthogonal volume. The angular momentum (H), the kinetic energy ($T$) and the torque of centrifugal forces (C) of a rotating rigid body are all in question, the last two taking the vector form $2T = \mathrm{H}.\mathrm{w}$ and $\mathrm{C} = \mathrm{H} \times \mathrm{w}$, w being the vector angular velocity.

H.—In Figure 1 let $m$ be any molecule of the body rotating clockwise about the $x$, $y$, $z$ axes and for convenience let the unit of time be infinitely small so that w may be infinitesimal. The figure shows the component angular velocities $\omega_x$, $\omega_y$, $\omega_z$, the corresponding tangential velocities $\omega_x r'$, $\omega_y x$, $\omega_z x$ and the




momentum couples per unit of $m$ $2a_1 = r'\omega_x \cdot r'$, $2a_2 = -x\omega_y \cdot y$, $2a_3 = -x\omega_z \cdot z$, where $r' = \sqrt{y^2+z^2}$ so far as displacements in a plane normal to $x$ are concerned. If therefore we make the summation for all the mass points $m$ of the body, w being constant, we obtain the coefficients $A - F - E$ of the first row of the tensor and the component moment of momentum about the $x$ axis, $A\omega_x - F\omega_y - E\omega_z$. The forces normal to $y$ and normal to $z$ contribute the other two, in turn.

H.W.—If this angular momentum about $x$ be construed as linear momentum relative to a radius 1 and if it be multiplied by the angular velocity $\omega_x$ also regarded as linear for the same unit radius, the product $(A\omega_x - F\omega_y - E\omega_z)\omega_x$ is twice the kinetic energy of the body, so far as rotation about $x$ is concerned. The other two axes make the corresponding contributions. The expression is interesting in showing how square product terms arise in the equation for kinetic energy.

H × w—In contrast to the preceding, the equation for the torque of the centrifugal forces is astonishingly complicated. I have analyzed it in Figure 2, to be interpreted in the same way as Figure 1, but referring to the torque about the $z$ axis. Centripetal force is generated by the rotation of a tangential velocity about a non-parallel axis (a few examples at the corners of Figure 2) and acts at $m$ with the appropriate lever arm here either $x$ or $y$. The tangential velocities $\omega_z y$ and $\omega_z x$ may be discarded; for either they are not rotated ($\omega_z y \cdot \omega_x$, $\omega_z x \cdot \omega_y$), or they generate pulls along $z$, $\omega_z y \cdot \omega_y$, $\omega_z x \cdot \omega_x$), while the torques $\omega_z x \cdot \omega_z \cdot y$ and $-\omega_z y \cdot \omega_z \cdot x$ balance. Since $\omega_x x$, $\omega_y y$, $\omega_z z$ have no meaning, there remain so far as $m$ alone is concerned, the tangential velocities (see Figure 2)

$$\omega_y x,\ \omega_x y,\ \omega_y z,\ \omega_x z$$

The per second rotation of these produce the centripetal forces per gram of $m$ (see figure)

$$-\omega_y x \cdot \omega_x + \omega_x y \cdot \omega_x - \omega_y z \cdot \omega_z$$
$$-\omega_y x \cdot \omega_y + \omega_x y \cdot \omega_y + \omega_x z \cdot \omega_z$$

which operate respectively with the lever arms $x$ and $y$, so that the full torque about $z$ is

$$m\,(-\omega_y x \cdot \omega_x + \omega_x y \cdot \omega_x - \omega_y z \cdot \omega_z)\,x - m\,(-\omega_y x \cdot \omega_y + \omega_x y \cdot \omega_y + \omega_x z \cdot \omega_z)\,y$$

To convert it into torque of centrifugal forces, these must be reversed. For symmetry $m\omega_y \omega_x z^2$ is to be added and subtracted, to match the terms in $x^2$ and $y^2$. Finally the summation is to be made for all the points $m$ of the body. The result (after arrangement) is for the axis $z$,

$$(A-B)\omega_x\omega_y + F(\omega_x^2 - \omega_y^2) + (D\omega_x - E\omega_y)\omega_z$$

with corresponding expressions for the $x$ and $y$ axis. Altogether therefore there are 18 deputy torturers engaged in their nefarious practices. Naturally if $D$, $E$, $F$, vanish, the treatment by the same method is simple.

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## THE SEAT OF FORMATION OF AMINO ACIDS IN PYRUS MALUS L.[1]

Although it has been established that nitrates can be reduced to nitrites in the roots and stems of plants, the possibility that the reduction of nitrites to form α-amino acids as an intermediate stage in the synthesis of proteins in plants could also take place in the cells of the roots and stems has not generally been accepted. Emmerling[2] indicated that in certain soils amino acids might be formed from nitrates in the roots and stems of some plants. However, Sachs (1865), Sorokin (1870), Pagnourd (1879) and Schimper[3] (1885) supported the photochemical theory of the formation of amino acids and proteins. They considered the chloroplast of the cells of the leaves maintained with a continuous supply of carbohydrates as being specially adapted to carry on the synthesis of proteins from ammonium salts and nitrates supplied to them by the conducting cells. Moreover, the rapid consumption of nitrates in the leaves is offered as the reason for the lower nitrate content in these tissues than in the roots and stems of some plants.

[1] Published by permission of the Director of the Agricultural Experiment Station as Technical Paper No. 425. Contribution No. 28 of the Department of Agricultural and Biological Chemistry.

[2] Emmerling, A., numerous papers in Landw. Ver. Stat. (1880–1898).

[3] See Czapek, F., Biochem. der Pflanzen 2: pp. 296–301 (1920) for a résumé.

This view that the synthesis of amino acids can take place in the leaves only has been especially emphasized more recently by Baly and his colleagues[4] from their experiments on the reduction of nitrates *in vitro*. It is maintained by these investigators that the activated formaldehyde $H \cdot C{\overset{\displaystyle /\!\!/O}{\underset{\displaystyle \diagdown H}{}}}$ produced photosynthetically in the living chloroplast cells reacts upon the first product of reduction (the nitrous acid salt—$KNO_2$) giving rise to formhydroxamic acid

$$\begin{array}{l} H-C-OH \\ \quad\;\; | \\ \quad\; NH \end{array}$$

, the formation of which was first established by Baudisch[5] by exposing a solution $KNO_2$ and methyl alcohol to ultra-violet light. This latter is assumed by Baly and his colleagues to react with another molecule of activated formaldehyde, giving rise to numerous nitrogen compounds such as, for example, the $\alpha$-amino acid—glycine. It follows, they claim, that the synthesis of nitrogen compounds *must* be restricted to the leaves. Although these papers have attracted considerable attention, plant physiologists have necessarily been cautious in accepting the conclusions drawn.

Thus, Dr. Eckerson[6] has shown that the hypothesis propounded by Baly *et al* that nitrates are reduced in the light by activated formaldehyde in green leaves is inapplicable to the results of her experiments on tomato plants having a high C:N ratio, since, in this case, when nitrates are fed, it is the fructose and glucose that are oxidized, accompanied by an hydrolysis of starch as the hexoses are used up in the formation of amino acids and a portion possibly in increased respiration. Suzuki[7] also obtained strong nitrate tests with barley plants fed nitrate only, the nitrate disappearing when sugar was added. The nature of the active material is unknown. Anderson[8] has postulated the presence of a reducing substance resembling the atite of Haas and Hill.[9]

During the investigations of the writer, extending over the past four years, on the nitrogen metabolism of Pyrus Malus, in which the partition of nitrogen has been studied in the various parts throughout a year's cycle, positive tests were found for nitrates (or nitrites) in one tissue only and this at just one period of the year, *viz.*, in the leaf buds just as they were opening. This work was carried out on mature and seedling apple trees receiving heavy applications of sodium nitrate at regular intervals throughout the vegetative period, by means of microchemical tests on sections of the leaves, tips of stems and one and two-year old branches with diphenylamine reagent,[10] "G" salt,[11] and the Griess-Ilosvay Reagent,[10] and also numerous *quantitative* tests[12,13,14] on both the dialyzed and undialyzed sap, preserved under toluene, and on aqueous alcoholic extracts of various tissues during the vegetative cycle. The fine roots gave nitrate reactions throughout the season; whereas in the main roots the reaction was much feebler and, as already stated, the tests were negative in the aerial parts except in the buds as they were opening. Correspondingly, quantitative tests for amino acids were always higher in the roots than in the aerial parts.

These results are in accord with the recent work of Dr. Eckerson,[15] who found that the reducing power of extracts from various parts of apple trees collected last September and November showed decided differences. The fine roots were very high in reducing activity, the buds less active and the bark of first and second year twigs had very little reducing power.

From the foregoing, it can scarcely be doubted that in this species the reduction of nitrates to amino acids takes place for the most part in the roots. Although experiments *in vitro* may be valuable in suggesting types of reactions that may occur in the cells of plants or animals, the extension of the results of such experiments as indicating the actual conditions existing to the processes *in vivo* should be made with caution. The internal conditions existing in the plant at any one time may bring about unlike chemical reactions to accomplish the synthesis of $\alpha$-amino acids and the different plant species may not carry out these syntheses in the same way. These investigations do not throw any light on the mechanism of the formation of amino acids in this plant and any suggestions offered at present would be purely hypothetical.

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